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SECTION OF GEOLOGY AND MINERALOGY

MAY 6, 1940

PROFESSOR BRADFORD WILLARD, Lehigh University: *Recurrent Paleozoic Continental Facies in Pennsylvania.*[1] (This lecture was illustrated by the use of maps and charts.)

All Paleozoic systems are represented in Pennsylvania. Commencing with the Ordovician, stratigraphic units of continental origin are known to occur and to displace marine units in each system. Omitting the Juniata of the Ordovician which is non-orthodox, the continental facies are successively the Jonestown (Ordovician), Bloomsburg (Silurian), Catskill (Devonian), Pocono and Mauch Chunk (Mississippian) and the Pennsylvanian and Permian systems. The earlier continental units are dominantly red, but from the Silurian on, non-red (greenish to gray) beds increase in proportion to the red so that by the close of the Paleozoic era, practically no red remains. Throughout this red to gray shift there is an alternation between the two types. It is assumed that the red beds are all syngenetically colored through derivation from reworked red regolith. Intervals of diastrophic quiescence of the land mass, Appalachia, afforded opportunity for such

[1] This paper will be published in full in the Annals of the Academy.

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regolith to accumulate. Uplift rejuvenated streams. With removal of the red cover, erosion cut away deeper, non-red, fresh rock. Such material deposited under fresh-water conditions became the non-red continental beds. Repetition of these stages is expressed in the alternation of red and non-red continental beds. The final dominance of the non-red is interpreted as an expression of the culmination of the Appalachian Revolution which marked the maximum uplift of Appalachia and its deepest erosion.

SECTION OF BIOLOGY

MAY 13, 1940

PROFESSOR ALAN BOYDEN, Rutgers University: *Serology and Animal Relationship.*[1] (This lecture was illustrated by lantern slides.)

Serology is that division of biology which deals with the nature and interactions of antigens and antibodies. Antigens are substances capable of inducing the formation of antibodies under appropriate conditions. Antibodies are substances which have the capacity of combining with and reacting upon the antigens which induced their formation and with chemically similar substances. In the present discussion the antigens used were the sterile sera of common mammals, amphibians and crustaceans. The antibodies (precipitins) were obtained by the repeated injection of particular sera into rabbits or fowls and the ultimate withdrawal of the antisera from the injected animals. These antisera were sterilized by Seitz filtration and stored in the ice box until used.

There are two great principles which apply to the comparison of the relative intensities of serological reactions:

1. An antiserum will react more strongly with the particular antigen used in its formation (homologous reaction) than with equivalent amounts of other substances (heterologous reactions).

2. The intensity of reaction between an antiserum and various antigens is proportional to the chemical similarity of the antigens tested.

These principles form the basic theory of all the applications of serological reactions, such as the precipitin test used in these studies, to the determination of animal relationship. What is done is to react each kind of antiserum, such as an antibeef serum, with beef serum (homologous reaction) and then with equivalent amounts of the sera of other related mam-

[1] Publication of the Bureau of Biological Research.

mals such as sheep, pig, horse, dog (heterologous reactions) and measure in some manner the relative intensity of reaction of each antiserum with the various antigens. Actually the results of such tests are measures of the chemical similarity of the serologically active constituents of the sera of the animals compared. This is, therefore, a literal interpretation of "blood-relationships."

Knowledge of animal relationship is of fundamental importance to zoologists. We mean by the term "animal relationship" an expression of the degree of similarity in the essential natures of the organisms compared. In so far as the characters measured are determined by heredity "animal relationship" coincides with "genetic relationship." Knowledge of such relationship is of fundamental importance to zoologists for these reasons:

1. Such knowledge is necessary for the development of a natural classification, *i.e.*, one which expresses the relative degrees of similarity in essential hereditary traits.

2. With such a classification generalizations can be drawn which are applicable to the members of natural groups and the transference of the conclusions drawn from experiments on particular species in the group to others in the same group is possible.

3. Knowledge of these natural groups can serve as a check on experimental results obtained with a particular species and as a basis for the determination of the relative importance of the experimental findings. For example, two investigators using one very similar, *i.e.*, closely related, species come to contrary conclusions as to the result of a particular kind of experiment. If both experimenters are right then the experimental findings are special cases and lack general validity. Or, either or both of the experimental results may be wrong and a repetition of the experiments is always indicated where there is lack of agreement in closely related animals.

4. Finally, knowledge of animal relationship, *i.e.*, genetic relationship, is necessary for our understanding of the nature

of evolution. The results of evolution are genetically related animals with varying degrees of similarity and differences. If we cannot ascertain which organisms are genetically related we do not know what the results of evolution have been, and, therefore, our understanding of evolutionary processes would be very inadequate.

If knowledge of animal relationship is so important, all possible means of determining such relationship should be employed. There are three chief divisions of Zoology used as bases for systematic analysis, (1) Morphology; (2) Paleontology; (3) Biochemistry. The first two of these sciences are well developed at present and have been used intensively for more than a century in systematic work. The last one and particularly that branch of biochemistry known as serology is much more recent and is, practically speaking, a twentieth-century development. In spite of long and intensive use of morphology and paleontology as bases for systematic work, we are still far from our goal of a truly natural classification and there seems indeed some confusion among taxonomists as to what the goal is and as to the general principles involved. As a matter of fact the grand object of classification everywhere is to group objects according to their likenesses. In biology the real goal of taxonomy is to group organisms in proportion to their degrees of resemblance in essential hereditary traits. It has often been stated that the goal of taxonomy is the expression of phylogenetic relationships. Actually the phylogeny is always inferred from the degree of similarity in characteristics believed to have genetic basis and a classification is not based on a phylogeny. Rather, both classification and phylogeny are developed from data regarding the essential, *i.e.*, hereditary natures of the organisms classified. It is true that we still have whole phyla of "uncertain systematic position" and nowhere have quantitative concepts been applied to systematic work with any great success. Some additional criterion for the determination of animal relationships which shall be entirely independent of morphology and paleon-

tology and which, it is hoped, will be more objective and more quantitative and less subject to convergence and environmental influence, is eminently desirable, to serve as a check on the other bases of systematic determinations.

Improved techniques for the performance of the precipitin reaction have been developed and the results of these tests examined to see whether serological comparisons can aid in the obtaining of the needed evidence. The ring test results on common mammals (beef, sheep, pig, horse, dog) gave values indicating the amount of serological similarity as obtained by testing an antibeef serum with beef 100%; with sheep 69.3%; with pig 13.2%; with dog 10.5%; with horse 9.4%. It should be pointed out that in the interpretation of these results the difference between the dog and horse values is not significant though each value is an average of several determinations. Nor may we conclude that because dog and horse are of the same apparent degree of similarity to beef that they are similar to each other. Each species must be compared with all the others by making antisera to each and testing these with all the other antigens. When this was done it was found that beef and sheep are most similar, that pig comes next and that horse and dog are least similar and about equally different from all the others. When plotted graphically 100% similarity means practical identity, *i.e.*, one locus on the family tree. Starting from the beef locus the sheep comes next with approximately 70% similarity or 30% difference which can be plotted as 30 units distance. Pig comes next with 13% similarity or 87 units distance from beef. Then follow dog and horse with 10% similarity or 90 units distance from beef. Taking all the values together a 3-dimensional expression was obtained showing beef and sheep close together, pig next and relatively distant, then dog and horse far from each other and from beef and pig.

Briefly the results roughly paralleled the systematic position of the species compared except that the horse and dog are not as distant from beef and sheep as they should be judg-

ing by the 87 unit distance between beef and pig, and by their morphological differentiation. In other words, the sera here are too conservative as between the orders Artiodactyla, Perissodactyla and Carnivora, in comparison with their differentiation within the order Artiodactyla, even though the pig does belong to a different sub-order of Artiodactyla (Suina) than the beef and sheep which are included within the sub-order Pecora.

Similar studies of common Amphibia in cooperation with G. K. Noble indicated the natural groups Caudata and Salientia as very distinct from each other and showed high similarity between *Rana pipiens* and *Rana catesbeiana*. Among the salamanders Cryptobranchus was used as a primitive genus and Necturus, Amphiuma and Siren were found to be fairly close to each other (12–22% distance) and much more distant from Cryptobranchus (83–97% distance). These data were of aid in establishing the position of Siren, which has been somewhat of a puzzle. The evidence indicated that Siren was not primitive but rather the farthest removed of the salamanders tested from primitive Cryptobranchus.

The results described above were all obtained by the use of the ring test and are, therefore, but crudely quantitative in nature. The procedure of measuring the volumes of precipitate obtained by the interaction of precipitins and various antigens was found to be too laborious for extended use. Recently an instrument for the measurement of turbid systems has been developed in our laboratories by R. L. Libby. This proves to be highly accurate and sensitive, capable of comparing the intensity of precipitin reactions in terms of readings on a galvanometer which registers the diffraction of light from turbid particles as it falls upon a photoelectric cell. It has been established by Libby that the "Photronreflectometer" readings are proportional to the amount of reacting antigen and antibody present.

With this promising instrument it is possible to distinguish between the sera of closely related animals and even to com-

pare the serological similarities of animals of known genetic relationship. For instance, a comparison of horse, mule and ass sera gave results indicating that the mule serum was between that of horse and ass and considerably nearer the horse. Antibeef serum gave heterologous reactions in the following order: bison, goat, elk, pig. A few comparisons have been completed using anti-blue crab serum which gave reactions in the following order: Callinectes, Carcinus, Cancer.

In all of the above Photronreflectometer comparisons the results are in accord with the systematic position of the species compared and with the genetic relationship where that is known. Without any further detail we may attempt a comparison of the relative advantages and disadvantages of the morphological, paleontological, and serological data upon which systematic classification may be based. Morphology has the advantage that it applies to all organisms and presents the greatest complexity of the character assemblage. On the other hand, its very complexity leads to difficulties in systematic grouping because different structural characters vary in their conservatism and it is difficult to summate the total degree of resemblance and reduce it to a simple expression. Morphological comparisons are often not suited to quantitative analysis and may be disturbed by convergence. Only paleontology can give us a rough chronology of some ancestral features, particularly those readily fossilized, *i.e.*, the hard parts of animals. It does not apply to all animals for whole phyla of existing animals such as the Ctenophora, Platyhelminthes, Nemathelminthes and major groups of other phyla are not and probably never will be adequately recorded as fossils. It is subject to the same limitations as morphology, besides its own incompleteness, but where the fossil history of hard parts is relatively complete, its evidence is decisive in phylogenetic analysis. As to serology it is better adapted to quantitative analysis, it is apparently free, or practically free, of convergence where proteins alone are concerned, it is least influenced by the environment and relatively constant during ontogeny.

These comparisons indicate that the data of these different zoological departments are in a real sense *complementary* for each one presents certain advantages over the others. On the same assumption that a more natural classification, *i.e.*, a classification which groups organisms according to their degree of similarity in essential hereditary traits, will result from the synthesis of all three types of data, we need to understand and appreciate more fully the peculiar contribution of each discipline and the possibilities of greater progress through mutual aid, toward the goal of a truly natural classification.

SECTION OF PSYCHOLOGY

MAY 20, 1940

PROFESSOR WAYNE DENNIS, University of Virginia: *Infant Reaction to Restraint: An Evaluation of Watson's Theory.*

Watson's theory of three native emotions, each with its specific stimuli, is so well-known that I shall not impose upon you to the extent of repeating it. Its neatness, its simplicity, its finality make it very satisfying. It has a brevity and conciseness which makes it as delightful as the statement that there are but six continents and three races, and it suffers from some of the same deficiencies.

The views of Watson concerning the emotions are often spoken of as if they constitute a closely knit structure. In point of fact, this is not the case. They may be subdivided into numerous theories, any one of which may be modified without greatly changing the remainder. If other native emotions were added to the list, or if some were subtracted, the general theoretical structure would not be much affected, just as one can add or subtract a continent or a race without altering one's general conception of the world or of the human race.

In this paper, I shall discuss chiefly but one of Watson's hypothesized native emotions; namely,—the rage reaction in response to restraint, but I hope that the treatment of this one response will show what is wrong with his scheme, and what will be wrong with any scheme of native emotions or native reaction tendencies which is based on the same principles.

It will be fruitful, first, to sketch the history of Watson's theory. That restraint of movement causes a decided reaction which may be called rage was first stated in 1917, in an article (29) entitled "Psychological Experimentation and Emotional Reactions." The theory is usually attributed to

Watson, although the original article was written jointly by Watson and Morgan. While Morgan assisted in the experimentation, he was not associated with the dissemination of the doctrine, and departed from it in later publications. However, for Watson, the theory of the native emotions which was first enunciated in the article cited above was not a view lightly held and easily relinquished. He presented it on many subsequent occasions. It appeared in a volume (15) entitled "Suggestions of Modern Science Concerning Education," published in 1917 and republished in 1921; in the Psychological Review, 1919 (25); in "Psychology from the Standpoint of a Behaviorist," of which the first edition appeared in 1919 (24) and the second in 1924; in the Scientific Monthly, 1921 (30); in the proceedings of the Annual Meeting of the International Kindergarten Workers, 1921 (26); in "Behaviorism," 1924 (28); and, in later editions, in "The Psychologies of 1925," (19), and in "The Psychological Care of Infant and Child," 1928 (27). The wording was practically unchanged throughout. In addition to its presentation in these more scientific works, it also found a place in popular journals. In view of this wide publication by a man who was very much in the public eye at the time, it is not surprising that Watson's theory became a matter of common knowledge nor is it surprising that it was incorporated into many textbooks in psychology and was accepted uncritically for at least a decade.

Watson's experimental contributions to the topic of native reactions may be said to have ended with his first article on the subject; that is, in 1917. All later publications were essentially restatements of his first findings, with no new factual contributions concerning infant emotional patterns. His research was interrupted by his participation in the work of the army during the World War and was ended by his departure from Johns Hopkins in 1920. In spite of his extensive writings, Watson never presented in detail the results of his infant research. He spoke of experimentation upon several hundred infants, but his reports are simply generalized

statements of his observations. This form of presentation gives an impression of stereotypy of infant reaction which he probably did not intend to convey.

The stimuli which Watson employed to restrain the infant were numerous. They included holding the head of the infant between the experimenter's hands or between pads of cotton, holding the nose, pressing the arms to the sides, and holding the legs tightly together. Watson repeatedly stated that the essential stimulus to rage was the "hampering of the infant's movements." He claimed that in no case was the pressure which was exerted sufficient to cause real pain, although this is a point concerning which only the baby could have had immediate knowledge. It is interesting to note the behaviorist taking such liberties with the infantile consciousness.

After Watson, the next experimenters who made use of similar stimuli administered to the newborn infant were Sherman and Sherman in 1925 (21). These investigators applied pressure to the chins of 83 infants. They found that diffuse arm reactions occurred; but the Shermans were more interested in the fact that in older infants the movements were better coordinated than they were in the fact that the reactions, as described by them, were somewhat similar to those which had been reported by Watson.

The later reports of Mandel Sherman (20) are widely known and frequently are cited in partial refutation of Watson's findings. Sherman, in a part of his experiments, exhibited to judges babies which had been stimulated in ways unknown to the judges. He also exhibited movies of infants reacting to stimuli which had been deleted from the films. One of his forms of stimulation consisted in holding the infant's head on the table "with fairly firm pressure." The point which was emphasized by Sherman was the fact that the observers did not designate with much correctness the causes of the reactions which they saw. This, of course, tells as much about the observers as it does about the infants. We do not know whether the reaction patterns of the infants or

the perceptual patterns of the observers were loosely organized. However, when the infants were restrained the observers almost invariably described the reaction as being of a vigorous character, and reported it as anger, fear, pain or colic so that it can be gathered that Sherman's restraint, like Watson's, resulted in pronounced reactions.

Next comes the monograph of Pratt, Nelson and Sun (18). A section of this publication deals with reaction to restraint. In one experiment the investigators "held the (infant's) arms pressed firmly against the body and held them there against whatever energy the infant would exert." However, in a tabular statement of the ensuing reactions one finds that sometimes the "arms immediately flexed to the original posture" and that sometimes the "arms did not remain at the sides of the body." One must conclude either that the infant occasionally overpowered Pratt, Nelson and Sun, or that these remarks refer only to what happened after the child was released. If that is the case, then the experimenters failed to state what happened while the infants were being held.

The only condition under which a sufficiently large number of subjects were stimulated by Pratt, Nelson and Sun was the state described as "asleep and dry." Watson, on the other hand, worked only with wide-awake babies, and it is likely that he did not intend his observations to apply to sleeping children.

In another part of their work, Pratt and his collaborators held the infant's nose between the thumb and forefinger with gentle pressure in such a way as to close off the nostrils. In some experiments this was done for 6 seconds, in others the stimulation was continued for 15 seconds. However, if the infant cried, the stimulation was immediately discontinued. In their typical style, these investigators report their results as follows:—To 234 experiments on infants who were asleep and dry, there occurred 197 movements of the extremities, 134 of drawing back of the head and 70 of arching of the spine. These experiments are often cited as being somewhat contrary

to the theory of Watson. If, however, it be conceded that Pratt, Nelson and Sun probably stimulated the subjects less vigorously than did Watson, if it be borne in mind that the condition of the infant when it was stimulated was different in the two investigations, if it be recalled that the duration of the stimulation was not the same, and finally, if one be reminded of the different manner in which the results were reported, it will be seen that there cannot be a contradiction between the two, since the second study scarcely in any sense was a repetition of Watson's. The most reasonable conclusion from Pratt, Nelson and Sun's work would seem to be that less severe stimuli cause less vigorous reactions.

Taylor's research (23) comes nearer to duplicating Watson's conditions for rage, but in this experiment, too, many of the infants were asleep. Furthermore, the stimulations, which consisted in cutting off inhalation by holding the nose between the experimenter's thumb and forefinger, or in holding the arms at the sides, lasted for only 20 seconds, whereas Watson, in personal correspondence with Taylor after Taylor's experiment was completed, suggested that a duration of one minute should have been used. In Taylor's investigation each of 40 infants was stimulated once by each of the two methods listed above. To nose stimulation, 6 subjects stiffened the body, 11 slashed with the arms, 30 abducted the head and 21 arched the back. To arm stimulation, 15 stiffened the body and 8 gave vigorous limb reactions. While Taylor concluded that he did not observe Watson's rage pattern, the correctness of this conclusion hinges upon what one means by a pattern. The responses which Taylor observed most frequently were a part of Watson's "pattern" but the pattern did not occur in its entirety at each stimulation.

I wish to report next some experiments which Mrs. Dennis and I performed upon a set of non-identical twins. Other researches upon these subjects have been described elsewhere (7, 8, 9, 11), but the following observations have not been previously reported. When the infants were about two

months old, they were presented with certain stimuli and their responses to these were recorded by motion picture camera. A series of stimuli was presented each day for 18 days. The stimulations included holding the head between the experimenter's hands, so that it could not be moved, and pressing the nose of the subject with the experimenter's forefinger. The amount of pressure involved in these two forms of stimulation was not measured objectively, but I can say that it was vigorous, since it was desired to obtain some responses to each stimulation.

In any form of experimentation one expects some variation in response from stimulation to stimulation, and this variation Watson's reports tended to ignore. In our records, the variability of the response is marked. Nevertheless, in approximately two-thirds of the stimulations, the movie records show behavior which may be characterized as struggling, thrashing about, or crying. This is not very different from Watson's so-called rage pattern.

But we not only stimulated the infants by holding the nose and by restraining the arms, but also by strong taste stimuli—a saturated salt solution, a very bitter quinine solution and by dilute citric acid. So far as we were able to determine, the limb reactions to these stimuli were not distinguishable from those which followed the stimulation by restraint. Let me call them by the neutral title of restlessness and crying. It is my belief that many strong and persistent stimuli will cause roughly the same sort of infant reaction.

Was Watson correct in assuming that it was *restraint of movement* which caused the restlessness and crying which he observed or was it caused by intense stimulation? Watson was not a gentle experimenter as anyone will attest who has seen Watson's research film which portrays his methods of infant investigation. He did not merely hold the infant's head and arms; he exerted pressure upon them. Pratt, Nelson and Sun, and Taylor, who merely restrained the movements of their subjects did not obtain such vigorous responses as did Watson.

In addition to the observations of the experimenters just mentioned, several other considerations suggest that restraint of movement of itself does not cause restlessness and crying. One of these considerations arises from the situation of the infant *in utero*. Here, if anywhere, is to be found a profound hampering of movements. The head, the arms, the legs, the trunk, all are restrained. There is no free space in which to move. The arms cannot be extended, the feet meet resistance in kicking, the head cannot be lifted from the chest. Yet the evidence indicates that the mature fetus moves only occasionally. From Watson's theory, one would expect the unborn child to be in a permanent state of violent rage. There is no indication that this is the case, and no indication that the prenatal responses which do occur are the result of restraint. Watson seems never to have faced the question as to why the infant immediately after birth should react emotionally to a condition very similar to that in which it exists for several months in relative quiescence.

If we consider the effective element in Watson's stimulation to have been the intensity of the stimulation rather than its hampering character, no such contradictions in interpretation are met. Intrauterine pressure is not great, and, since the fetus is in a fluid medium, the pressure is approximately equal upon all parts of the body. Each part is subjected only to a very gentle pressure.

No psychological theory ever revealed so clearly the need of psychology for an historical and cultural perspective as did Watson's hypothesis of a negative reaction to restraint in early infancy. Had Watson appeared upon the scene of Western civilization prior to 1750, he could not have formulated such a view, nor, in his own time, could he have done so if he had considered the methods of infant care prevalent before the modern era. The theory should have been forestalled by a knowledge of the widespread practice of swaddling and binding the infant. An adequate history of swaddling, so far as I can ascertain, has not been written. We know

that swaddling clothes were employed by the Jews, the Greeks and the Romans before the Christian era, and were used throughout the medieval period in all European countries. Swaddling did not begin to disappear in England until 1750, following a severe criticism of its detrimental effect upon infant health by an influential physician, Cadogan (3). Swaddling is practiced by the Lapps of today. I am told that it persists in some Italian peasant districts, and I have no doubt it is to be found in other parts of Europe as well. The Albanian custom is known through the study of Danziger and Frankl (6).

In its most extreme form, swaddling involved wrapping the infant round and round with long strips of cloth in a manner such that the arms were held extended along the sides and the legs were bound together. Furthermore, at times, the head was fastened to some sort of a stay in order to prevent it from wobbling. The general idea underlying swaddling practices was that of straightening the infant and of protecting him from injury. While the general nature of swaddling in Europe is clear from works of art and from casual references to it, I have nowhere been able to find a very adequate description of how it was actually done, or of how tightly the infant was bound. Salzman (19) states that the infant was able to move only its head; Crump (5) gives some description of the practice as does Buffon (2). The condition of the swaddled infant was such that, according to Watson's theory, the infant continually should have expressed rage. It has not been possible to find a single statement concerning the newborn infant's reaction to the swaddling process. Tiedeman, it is true, mentions a slight repugnance to swaddling on the part of his son, but this was when his son was several months of age. If children had uniformly cried during most of the time that they were swaddled that fact could hardly have escaped observation, nor, I think, could the practice have endured for centuries.

But we are not dependent upon European sources for information concerning reactions to swaddling. The binding of infants occurs in many parts of the world other than Europe. It was practically universal on the continent of North America at the time of its settlement by white men. The early explorers frequently mentioned the binding of the American Indian infant, probably because it differed in one or more respects from European binding. One of these differences consisted in the fact that whereas the European child was swaddled and then lain in a crib or on a bed, the swaddling of the Indian infant bound him to a rigid or semi-rigid cradleboard. The Indian infant was more restrained than was the European child. The accounts of various travelers, explorers, missionaries, do not indicate that the Indian infants objected to this treatment. In fact, many observers commented on the good behavior of infants when thus bound.

In some sections of America, many aboriginal customs still persist, and one can observe at the present time the reaction of the Indian infant to his bindings. I have been interested in making such observations among the Hopi and Navaho Indians of Arizona and New Mexico. Since these observations have been published in detail elsewhere (10, 12) they need be mentioned only briefly here. They show beyond doubt that the young Indian infant does not cry when bound, although his limb movements and bodily movements are very greatly restricted.

In the Southwestern culture area, the head of the infant remains free to move. But among some Indian groups, in the past, even the head of the infant was fixed. This condition was imposed as a part of the process of head-molding or head flattening, which was most prominent along the Northwest Coast, particularly among the Chinook. The procedure made the head completely immobile. At the same time, the limbs were encased by bindings. The flattening of the forehead, so as to make the head more or less wedge-shaped, was achieved by placing on the forehead a hard pad, which was pressed

against the forehead by means of the thongs which passed through the floor of the cradle and were tied. The thongs were kept tightened to what was considered to be just the right degree, the adjustment of them often being entrusted to skilled and experienced individuals. The resulting head shape was a mark of high social station and was considered to lend beauty. The process began shortly after birth and was continued, almost without interruption, for several months, often from 8 to 10 months. The head-molding technique has been described for various Northwest Coast groups by Kane (16), Swan (22), Cox (4), and Gunther (14). It is not followed at the present time.

Most of the writers cited above have said nothing about the behavior of the infant who is being subjected to this treatment. This, in itself, would suggest that the infant's behavior was not unusual. Bancroft (1) and Cox (4), however, stated that the infants seemed to feel no pain, and Kane (16) observed, "I have never heard the infants crying or moaning. . . ." Kane's interpretation, however, was that the infants may have been partially stupefied by the procedure. The eyes are described as bulging as a result of the intracranial pressure. The fact may be, however, that this pressure, although great, caused no pain. One can readily demonstrate to himself that pressure applied to the *forehead* does not hurt, whereas pressure applied to the sides of the head, where it was administered by Watson and others, is quite painful. The temporal regions of the head are particularly sensitive to compression.

The argument up to this point may be summarized as follows:—The newborn and slightly older infant has no unique pattern of reaction to restraint. He reacts to all strong and persistent stimuli with restlessness and crying, a crude pattern of activity which is similar for all sorts of intense and enduring stimuli. The effective aspect of the stimulations employed by Watson, Sherman, Taylor and ourselves was probably strong pressure or pain. On the other hand, methods which achieve restraint of movement without steep gradients of

pressure—such as the conditions which are found *in utero,* in swaddling, and in binding to a cradleboard—do not produce restlessness and crying. These statements, I believe, will harmonize all the seemingly contradictory evidence which has been presented.

Let us turn now to a consideration of the older infant. In Watson's theory, the negative reaction to restraint is characteristic not merely of the neonatal period but it is a permanent part of the human make-up. In "Psychological Care of Infant and Child" he stated that no amount of training will ever completely eliminate the response.

In later infancy, everyone knows that "thwarting" of various sorts will lead to something which may be called rage. The early baby biographies are full of instances of reaction to frustration. For example, it was recorded that one baby cried because he was not permitted to pull his grandfather's beard. Such stimulation is "restraint" only in a metaphorical sense. It is likely that Watson knew that such responses occurred, and his research may be interpreted as an attempt to find an instinctive basis for the responses to frustration. We have seen that he was probably wrong in tracing them back to the neonatal level. Rage, I believe, is a reaction which is not present until purposeful behavior has been developed and is foiled in attaining its end. It is unlearned in this sense, that when the child has developed a purposive sequence of behavior which can be interfered with, he will exhibit "rage" on the first occasion on which this interference occurs.

It was possible to observe early reactions to thwarting in the case of the two infant subjects previously referred to, since they lived under controlled conditions which made it possible for us to know when a particular activity first suffered interference. As soon as the infants became accustomed to playing with a rattle, which was not offered them until the eleventh month, each twin cried when her rattle was taken away. Another type of thwarting arose as follows:—When we picked up the infants, it was usually for the purpose of feeding them

or bathing them or experimenting with them. But occasionally we picked up an infant who was quiet and contented and returned her to the crib without further ministrations. In these circumstances she invariably cried. Furthermore, if we failed to experiment with either child at the usual time of day, she became restless. This may also be looked upon as a form of frustration, or at least as an interference with the usual routine.

Of course, physical restraint of movement may be employed as *one* means of frustrating the older infant. When our twin subjects were several months of age we restrained their arms by removing the arms from the sleeves of their garments, and buttoning the clothing over the arms so that they could not be employed as usual. This did not exert pressure but it made it impossible for the subjects to carry out their ordinary manual activities. This situation, of course, led to restlessness and crying.

Some Pueblo Indian infants at several months of age (10) begin to object to being placed on the cradleboard. But it is important to note that the Indian infant of this age has been off of the cradleboard each day for an interval sufficient to accustom him to manual play and to other activities. The negative reaction to the cradleboard which sometimes occurs in older infants may be due not merely to restraint but to the prevention of customary activities.

Physical restraint of movement does not *always* cause negative reactions in the older infant. In the case of the two infant subjects to which I have already referred several times, attempts were made to test reaction to restraint in the tenth month by repeating the head and nose stimulations which had been employed in the third month. No such stimulations had been presented during the interval. But, while Mrs. Dennis and I had not restrained the infants during this period, we had stimulated them in innumerable other ways. Not only had we fed, bathed, dried and clothed the subjects but we had plied them with odors, tastes, sights and sounds, as parts of

experimental tests. To most of these they had reacted favorably. In consequence, when one of us leaned over the crib and held the head of either subject very firmly between our hands, her reaction was to quietly look up and *smile*. For several minutes no negative reaction occurred. Eventually the child became tired of the stimulation, but no sooner than did we. It was sometimes necessary to hold a subject's head immobile for a full five minutes before she became restless and began to fret. I feel certain that it was discomfort rather than restraint which occasioned the final fretting.

It must be emphasized that the head and nose stimulations, in this situation, were not frustrating or thwarting in character. The infants were lying on their backs in their cribs, without toys, unoccupied. Their behavior indicated that they definitely wanted us to come near them. Any attention on our part, so long as it was not painful, they welcomed. Under the circumstances, their positive reactions to the head and nose stimulations were a form of positive social reaction. In other circumstances, holding the head or pressing on the nose might have been a pronounced form of thwarting, and might have led to different results.

Such considerations show the probable futility of attempts to test for "instincts" or native emotional stimuli after the very early months of life. Every stimulus which is presented, even if it has never before been employed, bears some relationship to the experience of the child. This is a fact which has general importance beyond the bounds of the mere topic of reaction to restraint. The child's reaction tendencies with respect to any test situation, like his reaction to restraint, can be stated, not absolutely, but only in terms of the relationship which the test situation bears to past situations and to the behavior repertoire. The child of several months of age does not react to a piece of fur, a snake, a loud noise, or a pinching of the nose, as a unique stimulus, as a thing-in-itself, but he seems to place each situation into some category of stimulation. A stimulation is a playful approach, a firm oral admin-

istration, a strange object, or a frustrating circumstance. The same absolute stimulating situation may be any one of these things depending upon the history of the child. Reaction tendencies beyond the first few months cannot be said to have absolute stimuli, such as loud noises or restraint of movement but instead they have relative stimuli, such as strange objects and frustrating situations (11). What is strange and what is frustrating necessarily depends upon the stimulation-history of the child.

It has been seen that reaction to frustration involves the prior acquisition of the response which is to be thwarted. Therefore, this reaction does not correspond to Watson's notion of an instinct; *i.e.*, a native response pattern elicited by certain native stimuli. The stimuli which will elicit rage are not determined solely by the structure of the individual but in part by the child's experience. In other words, a certain kind of learning is involved in reaction to frustration.

I would caution you, however, that this does not mean that this reaction is learned in the same sense that language is learned. In the past the gap between instinct and learning has been made too large. It has been customary simply to classify all behavioral phenomena with reference to this dichotomy, without attempting an analysis of the intermediate modes of development.

I believe a useful subclassification of the learned aspects of behavior can be attained by distinguishing between those things which the individual learns by himself without the aid or direction of others, and those things, like language, which are socially transmitted. The former, I have called autogenous, the latter, sociogenous behavior (11). It must be said that this division still is a crude one, but it is adequate for our present purposes.

Often we have been led by our terminology to place whatever is not totally instinctive into the class of socially transmitted phenomena. As we have accumulated evidence which indicates that stereotyped response patterns with predeter-

mined stimuli are fewer than we had supposed, we have put a great deal of emphasis upon the *social* determination of behavior. Today, no one will deny that a very large part of the behavior of the human *adult* is socially determined. But, in my opinion, we should not conclude that behavior during the first year of life also is socially determined. Most of the infant's repertoire, like his reaction to frustration, is attained through maturation plus the infant's individual experience, and does not require the intervention of other persons (11).

For it is true that the reaction to thwarting may come about autogenously. Our two infant subjects were under observation from the age of five weeks onward. In no sense can it be said that we trained them to cry when thwarted, or that we provided an example. Yet this response appeared without trial and error on the initial occurrence of each of several situations. In other words it was not a sociogenous response. To the contrary, the experiment proved it to be autogenous.

In closing, I may say that I have attempted to demonstrate the following points:—

1. At birth, the infant reacts with crying and restlessness to any form of intense and enduring stimulation, of which rough restraint may be one form.

2. Restraint of movement achieved without the use of intense stimulation does not cause negative reactions in the newborn.

3. At a later age, the infant will react also with crying and restlessness when some customary sequence of events meets interference.

4. Whether or not restraint of movement will cause negative reactions in the older infant depends upon whether or not the restraint interferes with customary sequences which have been built up. What is thwarting to the infant is not predetermined but depends upon post-natal events.

5. Interference with behavior sequences gives rise to negative reactions in the absence of any social transmission of this phenomenon.

Finally, I have suggested a briefer terminology, by which some of these views may be expressed as follows:—reaction to frustration is not congenital or instinctive but it is autogenous.

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SECTION OF PHYSICS AND CHEMISTRY

MAY 3 AND 4, 1940

Conference on "The Primary Process in Photochemistry."

Professor W. Albert Noyes, Jr., Department of Chemistry, University of Rochester, was in charge of this meeting as Conference Chairman. The program consisted of the following papers:

"Excitation of Asymmetric Vibrations During an Electronic Transition," by Doctor E. Teller, George Washington University.

"The Results of Photosensitization Experiments with Various Metals," by Doctor E. W. R. Steacie, National Research Council of Canada.

"Primary Processes in Fluorescence and Photosensitization with Special Reference to Simple Aromatic Compounds," by Professor W. West, New York University.

"Photolysis of Metal Alkyls and Their Significance in Photoprocesses," by Professor H. S. Taylor, Princeton University.

The New York Academy of Sciences

announces the

A. Cressy Morrison Prizes for 1940

The New York Academy of Sciences announces three prizes offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes, I, II and III, all of which will be awarded in December, 1940. Prize I, of $500, will be awarded for the best paper on solar and stellar energy as defined below. Prizes II and III will be awarded for the best papers on a scientific subject included within the field of The New York Academy of Sciences and its affiliated Societies. The terms governing these competitions are detailed herewith.

The competition for Prize I is open to all. Prizes II and III are limited to members of The New York Academy of Sciences and Affiliated Societies, but non-members may become eligible by joining one of these organizations before the closing date.

Prize I

A prize of $500.00 is offered for the thesis adjudged by the Council of the Academy to be the most meritorious on the subject formerly submitted for a prize and described on March 10, 1930, as follows:

1. *Subject:* "What may be proved from our present knowledge as to the possibility or impossibility of released intra-atomic energy constituting an important source of solar and stellar energy."

2. *Memorandum of Mr. Morrison:* With the offer described on May 5th, 1924, and renewed in 1925 and 1930, Mr. Morrison invited discussion of a memorandum submitted by him, hoping that such discussion would add to our knowledge of the subject. This memorandum is again submitted, without change.

"It is accepted that atomic matter is discontinuous. It is assumed that the atom is composed of a nucleus, like a central sun, with one or more electrons which are in extremely rapid motion with orbits at relatively large distances from the nucleus; that these atoms under certain conditions are capable of absorbing energy or giving off energy, and that the electrons may be changed from an outer to an inner orbit, or vice versa; these changes taking place when the atom is absorbing or giving off energy.

"With the condensation of a nebula to a sun, the ordinary sources of energy, such as contraction, meteoric showers, chemical changes, possible radium activity, and in fact all those well-recognized sources of energy which have been proposed, should be credited with their relative importance. However, beyond these sources it is suggested that under the conditions of temperature and pressure in the interior of the sun, the complex atoms are reduced to simpler atoms or disintegrated; and that energy is thus released from the atoms and constitutes the principal source of energy of the sun. The estimated life of the sun is therefore continued for an indefinite period, far in excess of any previous calculation, by the added amount of energy contained in the normal atoms themselves. The following thesis is proposed:

> 'The principle source of energy of the sun is intra-atomic energy existing within the normal atom itself which is released from the atom under the conditions of temperature and pressure which exist in the sun.'"

So far as is known, the theory presented, concerning the principal source of energy of the sun, had not previously been announced. A knowledge of the physical characteristics of the atom has advanced rapidly and the tendency of scientific thought toward the possibility of intra-atomic energy being released under the conditions of temperature and pressure which exist within the sun has gained credence. It has become apparent that the wide dissemination of the above thesis has led to the belief in many quarters that the thesis has been sustained and it is now being quoted in apparently authoritative quarters as orthodox astronomy. This somewhat general acceptance of the thesis has not, however, been warranted either by the papers which have already been awarded the prizes or by any recognized authority. However, it is equally true that no alternative suggestion as to the principal source of energy of the sun has been accepted by astronomers or physicists nor has the thesis been shown to be untenable. For this reason, this prize is again offered in the hope that the science of physics has sufficiently advanced since the thesis was first proposed in May, 1924, to enable science to fill the gap in our knowledge as to the principal source of energy of the sun.

An A. Cressy Morrison Prize on the above subject was awarded in December, 1926, to Donald H. Menzel of Lick Observatory, University of California. The same author, in collaboration with P. B. Gerasimovic of Harvard College Observatory, also won the prize awarded in December, 1928. In 1930, the prize was awarded to Professor H. von Zeipel, of the University of Upsala, Sweden, for his paper entitled, "The Evolution and Constitution of Stars" and the most recent prize was awarded in 1938 to Hans Bethe. The first paper was published in Science, May 26, 1927, and the second in Publications of the Astronomical Society of the Pacific, for April and June, 1929. It is assumed that with the rapid progress of astrophysical investigation much light will be thrown in the immediate future on the thesis advanced which it is the object of this competition to bring before the scientific world.

Prizes II and III

SUBJECT:

Two Prizes of $200.00 each are offered for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society. These papers must embody the results of original research not previously published.

CONDITIONS:

(1) Eligibility. Prize I is open to all competitors. Prizes II and III are open to all members in good standing of The New York Academy of Sciences or one of the Affiliated Societies, prior to submission of the manuscript.

(2) Date. Papers are to be submitted on or prior to November 1, 1940, to the Secretary of The New York Academy of Sciences at The American Museum of Natural History, Ceneral Park West at 79th Street, New York City.

(3) Papers. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication.

It is suggested that papers submitted for the prizes be accompanied by a sunmmary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the awards of a prize or prizes will be omitted for this contest.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1940 by The New York Academy of Sciences." This statement in substance must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

NEW MEMBERS

Elected May 6, 1940

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